

PARAPSYCHOLOGY AND ITS CRITICS:
IMPLICATIONS FOR PHILOSOPHY AND SOCIOLOGY OF SCIENCE*

A REPLY TO EDWARD GIRDEN

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Parapsychologists tend to overstate the case for psi. Hence, scholars who seek a balanced introduction to the field must familiarize themselves with critiques of the research. Unfortunately, there are no careful critiques that adequately survey the entire field. C. E. M. Hansel's ESP: A Scientific Evaluation (1966) is often cited in this regard, but this book dealt with only a few experiments and relied almost exclusively on an a priori assumption that psi cannot occur. Edward Girden's 1962 Psychological Bulletin review of PK research is another frequently cited source. Although Girden did raise some legitimate criticisms, he spent most of his attack on minor experiments that no one had ever cited as evidence of psi. In addition, Girden employed polemics that were rather far removed from careful scientific argument.

In a more recent critique (Vol. 10 of the Handbook of Perception), Girden attempts to show why parapsychology has failed to gain scientific acceptance. To Girden, the answer is simple. The parapsychologists are a group of credulous pseudoscientists, whose views are in conflict with all of established science. They are "true believers," who refuse to acknowledge evidence of fraud, even when the evidence amounts to conclusive proof.

Perhaps to correct for this, Girden makes his own accusations of fraud, without, however, providing much supporting evidence. Thus, he asserts that fraud was "unequivocally established" in three classic experimental series: Pearce-Pratt, Pratt-Woodruff and Soal-Goldney. But the critics whom Girden cites never made so strong a statement. Hansel claimed only that the Pearce-Pratt series did not exclude the possibility of fraud (of which there was no evidence). In their analysis of the Pratt-Woodruff experiment, Medhurst and Scott (JP, 1974, pp. 163-84) claimed only that fraud was a probable explanation of the obtained results.

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In the case of the Soal-Goldney experiments, Scott and Haskell (PSPR, 1974, pp. 43-111) did claim to have made "rather a strong case" for fraud. The continuing controversy over Soal's research led to Markwick's recent investigation (PSPR, 1978, pp. 250-78), which provided more definite evidence of fraud. If parapsychologists fitted Girden's profile of the "true believer" they should by now have either ignored Markwick's findings, or found some devious way of avoiding her conclusions. But this has not been the case. Even Soal's coexperimenter, K. M. Goldney, has acknowledged the strength of the evidence implicating Soal.

Girden's general contention is that parapsychologists tend to ignore or cover up allegations of fraud. In support of this thesis, Girden somehow manages to construe Rhine's exposé of Levy (JP, 1974, pp. 215-25) as a defense of a fraudulent experimenter. Rhine is portrayed as having defended all of Levy's work, prior to the exposé, as "authentic." But Rhine never made such a foolish statement. The adjective that Rhine used was "unacceptable," and he applied it to all of Levy's work, "published or unpublished, authored by him alone or jointly with others."

In another unsupported allegation, Girden refers to "pertinent sources of error" in a study by Fisk and West. The errors were supposedly suggested by the subject of the study, a Dr. Blundun. But in the source cited by Girden, there is not even a passing reference to the Fisk and West research.

If parapsychologists are inclined to overstate the case for psi, Girden seems determined to totally misrepresent it. Thus, he blandly asserts that Schmidt's findings have never been replicated. He also implies that there was not a single successful replication in the Stepanek research. Yet, the work with Stepanek (PASPR, 1973) represents an impressive series of strict replications, which were often carried out by visiting scientists who brought their own test materials.

REPLY TO PERSI DIACONIS

Edward F. Kelly (Duke University)

Persi Diaconis' recent critical attack on parapsychology research contains numerous major errors and misrepresentations. Referring primarily to anecdotal and non-experimental material rather than the actual experimental literature of the field, he repeatedly characterizes as typical of current research, situations and procedures which are in fact very atypical. He makes no effort to document sweeping and inaccurate generalizations about alleged inadequacies of the research, and he makes numerous incorrect and misleading statements about specific matters of statistical and experimental fact. Undisciplined attacks of this sort can only obstruct the development

of informed and rational discussion of this still controversial subject. Fortunately this and related issues will soon be explored in detail in the forthcoming volume, Science and Psi: The Pro and Con Debate (McFarland) edited by K. R. Rao.

WHENCE THE ENCHANTED BOUNDARY: CULTURAL SOURCES OF INTOLERANCE FOR PARAPSYCHOLOGY

Brian Mackenzie[†] and S. Lynne Mackenzie (University of Tasmania)

Parapsychologists sometimes claim that critics of the field are harsh and intolerant, carrying skepticism to the point of irrationality. W. F. Prince's The Enchanted Boundary is a well-known collection of instances in which critics, having stepped over the enchanted boundary separating parapsychology from ordinary science, lose their impartiality, fairness and critical acumen. Can such a tendency be explained? Sometimes it is suggested that overweening prejudice, either materialistic or religious, is what makes critics so hostile. The implication is that some disturbing influence robs critics of their usual rationality, and that if not thus disturbed they would discuss parapsychology in a much more balanced way.

We suggest, however, that this critical intolerance for parapsychology has deeper roots, grounded in the assumptions underlying the modern western confidence in rationality itself. Critics are often intolerant of parapsychology because it seems to be undermining these assumptions, undermining the conviction of rationality itself. Furthermore, we suggest, the critics are quite correct; that is just what parapsychology does.

The substantive assumptions central to the modern western confidence in the power of reason are embedded in the beginnings of the seventeenth century scientific revolution. The fundamental one, explicit in the writings of Galileo, Descartes and others, has been called the "reification of mathematics," the conviction that the world can be completely understood through, and only through, mathematics. A corollary is the distinction between primary and secondary qualities, whereby directly measurable and hence "primary" properties of objects (length, size, etc.) are physically real, while not directly measurable and hence "secondary" ones (color, smell, etc.) exist only in the mind of the observer. The distinction is necessary to permit the field of applicability of mathematical knowledge to be coextensive with the real world. As a result the secondary qualities, and by extension any other aspects of experience that could not be assimilated to a mathematico-physical view of nature, came increasingly to be considered separate from the physical world. They lacked physical reality and had, at most, a real status only in the mind. In this way both the methodological and the theoretical basis of the scientific revolution required an a priori conception of the world as a self-contained mathematico-physical

system, in which irreducibly mental qualities had a physically indescribable position, tolerable only if they were confined within individual organisms. This a priori conception was a condition of the intelligibility of nature.

Parapsychological phenomena are a priori impossible because they violate these a priori assumptions necessary to guarantee the intelligibility of nature. They depend on the causal efficacy of putatively irreducible mental or otherwise nonmathematico-physical factors outside the boundaries of individual organisms. It is irrelevant whether these irreducible and causally efficacious elements exist in some way separate from individuals (e.g., Mesmer's universal fluid), or wholly within individuals but with extra-individual causal significance (e.g., many accounts of telepathy). In either case, things that cannot belong in nature are acting in nature. Parapsychology and its forerunners thus constitute an affront, not primarily to particular scientific theories (which are modifiable), but to the common foundation of scientific theories, scientific method, and the enlightened commonsense view of the intelligibility of nature.

Serious more or less empirical involvement with the paranormal on this definition has flourished mainly since the Enlightenment, comprising what we label the "parapsychological tradition." It has largely been a direct and continuing reaction against this exclusion of uniquely mental or otherwise physically irreducible factors from the "real," including the physical, world. The parapsychological tradition has differed significantly from the religious, philosophical and mystical traditions, which have also frequently been incompatible with aspects of the scientific tradition. The first two were able to make gradual accommodation to the claims of science, maintaining their authority in a restricted domain. The third avoided detailed confrontation by basing its opposition largely on a monolithic world-view, incommensurable with the scientific one.

However, the parapsychological tradition has typically insisted that its claims are both methodologically and theoretically relevant to the scientific tradition, and has demanded substantial revisions to the latter. It has thereby been committed to detailed ongoing opposition to parts of the scientific tradition, as well as to the assumptions on which it is based and which have formed in turn the basis for a general cultural confidence in the power of reason. Under the circumstances, it is not surprising that parapsychology and its forerunners have often received apparently harsh treatment at the hands of critics. What may seem more surprising is that, with this background, it should ever have received any sympathetic hearing at all.

On the other hand, the depth of the parapsychological tradition's opposition to the assumptive basis of modern science guarantees that, should it become able to force acceptance of its claims, the achievement would have very great implications for both methodology and theory in the scientific tradition generally, as well as for their cultural offshoots. Unfortunately for the modern proponents of the parapsychological tradition, they have not been able so far to coerce

any such acceptance of their claims; since all attempts to do so face the evaluative resistance stemming from the a priori impossibility of such claims being valid, it is not clear what sort of demonstration would be sufficient for the purpose. But unless (or until) such an achievement is realized, the a priori unacceptability of parapsychology is unlikely to be significantly reduced, and parapsychologists should not expect, despite individual exceptions, to receive much more sympathetic general treatment from their critics than they have received up to now.

NORMAL EXPLANATIONS OF THE PARANORMAL: THE DEMARCATION PROBLEM AND FRAUD IN PARAPSYCHOLOGY

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This paper examines the problem of demarcating genuine science from "pseudo science." It is shown that it is possible to turn the demarcation arguments which have been used against "pseudo sciences," such as parapsychology, against the fraud hypothesis--which is the principal normal counter-explanation for the parapsychological evidence. It is argued that the fraud hypothesis fails to be scientific on the grounds of replication, metaphysical bias, falsifiability and lack of theory. Since fraud is accepted and parapsychology rejected, the role of demarcation criteria in determining acceptable science is challenged. An alternative account of their role is presented. It is argued that the rejection of parapsychology rests on cultural differences which demarcation criteria serve to legitimate.

ON "THE SCIENTIFIC CREDIBILITY OF ESP"

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In a paper entitled "The Scientific Credibility of ESP" Moss and Butler reject the case for ESP on six grounds. 1) The test procedures are so inadequately reported and the experimental designs are so informal that the evidence generated by parapsychological experiments cannot be regarded as establishing the existence of ESP. 2) Replication by a qualified nonsympathetic observer is essential before results should be accepted, and no such replication has been successfully carried out in parapsychology. 3) In order to believe in ESP we must discover at least one lawful relationship involving ESP, and there are no supposed "ESP laws" that cannot be accounted for more parsimoniously by existing psychological laws. 4) ESP is not in harmony with established laws and therefore it must be rejected. 5) We do not encounter ESP in the market place, therefore it must be spurious. 6) There is no need to have an open mind on the question of ESP if the evidence has not yet established it.

It is shown 1) that the reporting style and experimental design of good experiments in parapsychology are just as good as any in the behavioral sciences; 2) that replicability cannot be a primary criterion for demarcating the genuine from the spurious in every controversial area and that, in any case, parapsychological findings have, in a significant sense, been replicated; 3) that failure to find lawful relationships does not logically negate the existence of a phenomenon and that, in any case, there is sufficient evidence in ESP data to suggest such relationships; 4) that lack of perceived harmony with the "established laws" does not warrant the rejection of evidence and that, in any case, it is by no means certain that psi phenomena are outside the scope of all physical laws; 5) that the "marketplace" test is irrelevant to the question of the existence of a phenomenon if it fails; and 6) that the evidence for ESP is strong enough to compel an unbiased observer to take it seriously.

Moss and Butler argue that a) "the discovery of at least one lawful relationship involving ESP which cannot be explained more parsimoniously by an already existing psychological law" (p. 1069), is a precondition for the recognition of ESP and that b) this condition is not satisfied by the available evidence in parapsychology. This argument is fallacious on both logical and factual grounds. First, while the discovery of a lawful relationship involving a phenomenon logically entails the existence of that phenomenon, failure to discover such a relationship does not necessarily negate its existence. Second, Moss and Butler are incorrect in their assertion that there are no lawful relations in parapsychology, if we mean by lawfulness a degree of generalizability of results. A number of known relationships exist. It is recommended that anyone interested in a review of the experimental results bearing on this question read the Handbook of Parapsychology edited by Benjamin B. Wolman (1977).

Moss and Butler conclude that ESP "is nothing more than a thinly disguised form of essentialism, a reversion to a prescientific religio-mystical tradition. It relates, quite clearly, to the primitive practice of assigning causation to mysterious, impalpable, evanescent inner forces whenever the natural web of causation is not immediately apparent ... The deleterious effects of ESP beliefs may be obscure and delayed, but they are real and inevitable. The direction of human affairs based upon misconceptions must, in the long run, produce maladaptive and antisocial effects" (p. 1077). In the same blatantly rhetorical vein, it might equally well be said:

Rejection of ESP evidence is a cleverly disguised form of naive materialism on which totalitarian systems are founded. It relates to the least defensible but most widely used form of repression of new and challenging ideas, a denial of anything that does not conform to the orthodoxy and the establishment, a tendency to reject a phenomenon whenever a cause-effect chain is not immediately apparent. The deleterious effects of such a philosophy are all too obvious in the history of civilizations. Moreover, it is a philosophy that cuts out the

roots of the values which we in free societies so solemnly cherish.

Of course, such arguments neither establish nor refute ESP; they only arouse sentiments which help further to confuse the issues. In the final analysis, the case for or against ESP rests with the quality of evidence one way or another. But the quality of that evidence can be judged best in an atmosphere in which the effects of prejudice and bias are minimized by strict application of scholarly discipline. The paper by Moss and Butler does not, in my judgment, contribute to informed scientific discussion of parapsychology. As a mixture of rhetoric, dubious philosophy of science, and false and misleading statements about particular matters of experimental fact, it may reinforce a skeptic who already had made up his mind and misinformed someone who is unaware of the state of parapsychology, but it hardly contributes to an objective and scientific settlement of the disagreements concerning psi.

"PSEUDOSCIENCE? OR PSEUDOCRITICISM?"

Theodore Rockwell (Chevy Chase, Maryland)

Legitimate scientific criticism is seldom accorded psi research by other scientists. Most criticism of the field has little to do with the subject. The critics usually describe a variety of folk beliefs (such as Bermuda Triangle and Astrology), cite fraudulent 19th-century spiritualists and more recent stage figures and leave the impression that their remarks apply to current research--which they have not discussed. Critics who do discuss the subject still take an entirely different approach from what they would apply to other areas of research. They assume, a priori, that psi phenomena do not exist, and thus the events described by the investigator must not have occurred. This leads to conclusions of fraud or self-deception, postulation of fantastic scenarios to avoid psi, cries for "tighter controls" imposed by a board of hostile magicians and finally to demands that psi research be "thrown out of the workshop of science" until "battle-tested evidence" is in hand.

Such pseudocriticism serves neither psi research nor science. It reveals a fundamental ignorance of both. The charge that psi research is not a science is semantically invalid; science is a process, not a subject matter. Science is not defined by fields of research but by techniques and approach--the "scientific method." This method may be applied to any postulated or apparent phenomenon, and if it is done competently, the investigation is properly called scientific.

The experiment is only a part of the chain of processes called science. Scientific "truth" is a temporary consensus arrived at by continuous interaction among scientists. This interaction is brought

about by publication. Therefore, a critical part of the scientific process is the procedure by which scientific papers are reviewed for publication. A similar process is used to evaluate research proposals for funding. This process, called "peer review," depends on the judgment of scientists knowledgeable about the field. Mal-operation of this part of the scientific process is even more harmful to science than a poorly run experiment, since it can introduce a continuing bias not subject to the natural correcting forces in the process. It is therefore suggested that all scientists feel and exercise a responsibility to examine and publicly criticize aberrations in the review processes with the same zeal they apply to reports of experiments.

Examples are given of rejection letters and reviewers' comments that deserve condemnation in their own right, regardless of any lack of merit in the papers being reviewed. It is suggested that this situation results from an a priori premise by some editors that peer review will not work in this field, and that special steps must be taken to find reviewers who do not "believe in" psi phenomena. Such a position is without merit and demeans science. If one believes in science, one will trust it to determine the truth concerning psi or any other phenomena in the physical world.

CRITICISMS OF PARAPSYCHOLOGY

Douglas M. Stokes (Avon, Connecticut)

Recent criticisms of parapsychological research by Paul Kurtz and H. B. Gibson have appeared in the Skeptical Inquirer and the Bulletin of the British Psychological Society, respectively. Both critiques cite examples of alleged sloppy experimentation and experimenter fraud in support of their cases. They also point to the lack of a repeatable psi experiment as evidence that the parapsychologists have not established their claims. Both authors remark that phenomena that have not been explained at a particular time (such as the bat's navigational abilities) are often asserted to be inexplicable in principle and hence "paranormal" by some researchers. They provide a psychological analysis of the reasons why parapsychologists may succumb to the temptation to resort to fraudulent practices, including the desire to uphold a religious or spiritualistic view of the world (Kurtz), the desire not to be seen as being taken in by a pseudoscience and the desire to further the scientific and sociological status of parapsychology (Gibson).

Several errors occur in these two critiques. Kurtz asserts that PK necessarily contradicts the law of the conservation of energy, whereas it need not do so under the modern observational theories (such as those of Schmidt and Walker). Kurtz suggests that psi-hitting and psi-missing may well average out to chance, but he neglects to note that a psi effect would still be detectable through a

variance analysis. He criticizes the vagueness of parapsychological theory in not distinguishing between PK and precognition. What this amounts to is a criticism of parapsychologists for their caution and desire not to theorize unduly beyond their data. He also grossly misspells the name of Eusapia Palladino (he calls her Eustasia), suggesting that he is criticizing a field with which he is not entirely familiar.

Gibson's errors are more errors of judgment than of fact. He uses the fact that Ingo Swann linked the SRI research to the Scientology movement (in a way which Gibson does not specify) to discredit these experiments, a guilt-by-hearsay-association tactic. He also states that the SRI research gives the "kiss of death" to parapsychology in that the experimentation was so good that fraud must be inferred! (This directly contradicts Kurtz's use of the SRI research as an example of sloppy experimentation.) He states that the accusations of fraud in parapsychology reflect badly on science in general and that it would have been better had scientists never entered the field. The implicit assumption that, were it not for parapsychology, science would be a fraud-free enterprise is an absurd one for anyone even minimally conversant with the history of science.